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A RE-EXAMINATION OF BANVILLE AND LANDRY'S "CAN THE FIELD OF MIS BE DISCIPLINED?"

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A RE-EXAMINATION OF BANVILLE AND LANDRY'S "CAN THE FIELD OF MIS BE DISCIPLINED?"

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Abstract

This paper takes on a contrarian view of Banville and Landry's (1989) landmark article "Can the Field of MIS Be Disciplined?" Notwithstanding its valuable contribution to the discussion concerning the progress of the IS field and its considerable influence on IS research, the article's interpretations of Thomas Kuhn's ***The Structure of Scientific Revolutions*** *and proposed model for the IS field have not been critically analyzed. Inspired by Richard Whitley's (1984)* ***The Intellectual and Social Organization of the Sciences****, the landmark article argues that Kuhn's model for scientific progress is vague, rigid, totalitarian, monistic and therefore unsuitable for a multidisciplinary field like IS. A critical analysis of the article's claims and arguments finds several uses of rhetorical devices in their interpretation of Kuhn's model which prevent the IS community from taking advantage of the paradigm concept and engaging its multidisciplinary roots. Practical implications for a more disciplinary IS future are proposed.*

Keywords: Information systems (IS) Philosophy, IS research, IS theory, paradigms, Kuhn, normal science

Introduction

This paper is not an exposition on the disciplinarity of the IS field, or an argument for or against the IS field having a paradigm or theoretical core. This paper is about the legacy of a landmark article in the *Communications of the ACM* that triggered, justifiably or otherwise, the ongoing debates on the identity, disciplinarity and lack of theoretical core of the IS field. Google Scholar reports over 270 publications citing Banville and Landry's (1989) "Can the Field of MIS Be Disciplined?" (henceforth BL), making it an IS super classic (Price 1963). Its characterization of the IS field as a "fragmented adhocracy," often without the accompanying citation, is probably referenced many more times over. It would not be an overstatement to say that this article has not only shaped the direction of research in IS, it has in many ways molded the nature of the field. Despite its considerable influence and impact, its claims and prescriptions have not undergone any critical analysis. Gordon Davis (1979), considered the father of the IS field, recommends that IS researchers constantly examine generally-accepted but unproven suppositions or weakly-proven assertions in the field. In light of this article's considerable impact on the IS field and following the spirit of Gordon Davis's recommendations, this essay critically reexamines BL's assertions.

BL contributed valuable insights towards the discussion concerning the progress of the IS field. It highlighted a growing preoccupation within mainstream IS research for constructing the perfect IS framework or discovering the single grand theory that characterizes the IS field. The article also drew the attention of the IS community to the significance of the Kuhnian model of scientific progress, albeit in a negative way. It emphasized not only the need to understand the importance of the disciplinary and scientific status of the IS field, but more importantly, how the IS field is to realize its disciplinary goals and scholarship. Is the IS field supposed to emulate the pattern of progress shown by the natural sciences or is it closer to the more pluralistic social sciences? BL categorically favors the latter and argues against the "imposition" of any natural science paradigm over the IS field. In order to justify their claims, they chose to discredit not how the IS field conceived of the Kuhnian paradigm or how it is manifested in IS research; instead, they chose to discredit the Kuhnian paradigm concept itself. This paper examines their arguments

and sources and evaluates the legacy that the paper has left within the IS field. As a result of this examination, the paper suggests several implications towards the future for a more disciplinary IS field.

Methodology

The examination of BL is undertaken by combining an exposition of the Kuhnian paradigm with the argumentative method of Stephen Toulmin (1958). A brief description of the Kuhnian model of scientific progress is given in the next section to provide the necessary background for the Toulminian method that follows it. This description emphasizes the sociological and historical approach taken by Kuhn to explain the evolution of the sciences. The Kuhnian model is then contrasted with the claims made by BL, in particular, that: (1) the paradigm concept is too vague and diffused, (2) the paradigm concept is rigid, monistic and unsuited for a pluralistic field like IS, and (3) the sought- after "normal science" phase of the Kuhnian paradigm model is prosaic and offers little to the IS field. For each claim, the source of the evidence and their associated warrants are examined in detail. Attention is especially given to BL's main reference: Richard Whitley's (1984) *The Intellectual and Social Organization of the Sciences*. Finally, in the conclusion, practical implications for a new future of IS stemming from this analysis and future research possibilities are proposed.

The Kuhnian Model of Scientific Progress

The main premise of BL is founded on a repudiation of Kuhn's (1970b) model of scientific progress; therefore, a brief review of Kuhn's model is in order. The years before the publication of BL reflected a strong sense of ambivalence, bordering on dissension, towards Kuhn's model in the *Structure of Scientific Revolutions* (henceforth *Structure*) from philosophers the likes of Karl Popper (1970) and Stephen Toulmin (1970). At the same time it also received much praise and support for what some say was the "best-known academic book of the second half of the twentieth century" (Fuller 2000, p. 1). The notions of differing paradigms and worldviews described in *Structure* were received with a sigh of relief especially by social scientists who had struggled for decades under the hegemony of the positivist "hard sciences." The social sciences were the first to latch on to the ideas propounded by *Structure*, and even though they were unable to agree on many fronts within their own fields, quickly declared respectability under the protection of their own unique paradigms. What was amazing about *Structure* was how Kuhn was able to persuade the natural scientists of the validity of his radical ideas using evidence from their own fields, radical ideas that coincidently were commonplace among social scientists (Fuller 2000). New fields of study such as the "Sociology of Knowledge" (Merton 1973) and "Science Studies" owe a large debt to *Structure* (Barnes 1982).

The reason why *Structure* was met by so much opposition in the beginning was because to some it undermined the authority of science, and in a scientific world, its propositions were nothing short of blasphemous. Science, which was believed by many, to be a logical and rational enterprise was turned upside down by Kuhn, who suggested that science is *nothing more than what scientists do*. For Kuhn, scientists perform and evaluate research in reference to rules, exemplars and models that all of them acquired during their training. This conduct of "normal science" reflected the vast majority of scientific work, which although carried the progress of science forward, often obscured the scientists from seeing beyond their existing world-views. These changes of world-view or "paradigm shifts" resembled "revolutions" in scientific progress, though rare, represented instances of extraordinary scientific discoveries. From this insight, the term "paradigm" grew to mean many things to different people. Kuhn injected a sociological perspective into the traditional view of science implying that even the most rigorous natural scientist is merely part of a community that is equally open to ideological strife and controversy. Consequently, not only was scientific objectivity put on trial and exposed to a level of scrutiny it did not experience previously, Kuhn's work suggested that any activity could claim scientific status as long as it is coherent within its own world-view (Fuller 2000). As Masterman (1970) puts it, "we are not going to be able to go back to where we were before Kuhn" (p. 87).

Among the earliest to disagree with Kuhn was the "realist" camp, represented by Karl Popper, that views reality as existing independent of observation, not as a world-view. As such, the progress of science is governed by how well we fit our conjectures and theories to this given reality. Popper (1970) considers Kuhn a "relativist" (despite his protests) because Kuhn "suggests that rational discussion, and rational criticism, is only possible if we have agreed on fundamentals" (p. 56). This "incommensurability" results in a situation where perceptions of reality differ between groups depending on their world-views. Popper maintains that science progresses as a result of accretion in knowledge, as better theories falsify weaker ones, and considers Kuhn's claims to be psychological rather than logical. Lakatos (1970) agrees with Kuhn's criticism against Popper's naïve falsificationism, but disagrees with

Kuhn's total rejection of falsificationism and considers Kuhn's model as irrational. Instead, Lakatos prefers a sophisticated version of falsificationism involving the testing of a "series of theories" in a heuristic manner within research programs. Most of Kuhn's critics focus on three major elements of Kuhn's model, (1) the paradigm and normal science concepts, (2) his purported relativism, and (3) the magnitude of the "revolution" or change in scientific progress. Toulmin (1970) for example, disagrees with the use of the term "revolution" in scientific progress and instead describes the progress of science in terms of a conceptual *evolution* instead of a revolution of human understanding. Many critics disagree with Kuhn's notion of "normal science" claiming that it either does not exist or at least demeans the integrity of the scientific method (Watkins 1970).

A deeper reading of Kuhn's critics however shows that they agree with Kuhn more than they care to admit. For example, although Toulmin (1970) disagrees with Kuhn's use of the term "revolution," he is in complete agreement with Kuhn's concept of the scientific community as "men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors" (Kuhn 1996, p. 177). Toulmin (1972) bases his definition of disciplines on the same "scientific community" which shares the same "agreed, communal goals" and paradigm in the sense of "an agreed direction of conceptual and procedural change" (p. 359). The subtle differences between Kuhn and his critics obscure the larger agreement they share, and the dialog between Kuhn and his critics contributed greatly to a better overall understanding of the progress of science. This larger agreement, and the correspondingly smaller disagreement, becomes more obvious when the individual components of Kuhn's theory are considered together in relation to one another. In many ways, the misunderstandings of Kuhn's theories reflect the inability of the discussants to intersect on issues causing severe communication difficulties, much like how it is described by Kuhn's theory (incommensurability) itself.

Will the Real Kuhn Please Stand Up!

Kuhn's theory was given so many varied interpretations, and in some cases outright distortions, such that Kuhn (1970a) himself felt, in replying to his critics, that there were two Thomas Kuhns, the Kuhn replying to all his critics and the Kuhn who first wrote *Structure* in 1962. All of these varied interpretations prompted Paul Hoyningen-Huene (1993), a physicist and philosopher from the University of Hannover, to reconstruct Kuhn's thoughts and philosophy in the effort to clarify any obscurities or ambiguities in the writing itself as well as its readers. Hoyningen-Huene's unique reconstruction and the numerous other appraisals and reviews (Barnes 1982; Fuller 2000; Gutting 1980) form the backdrop for this paper's re-examination of BL

A detailed re-examination of BL exposes these same misunderstandings and more. The bare summary of Kuhn's *Structure* offered above provides a preview of several of these misunderstandings. Kuhn's critics accuse him of being a relativist, which, in essence amounts to being a pluralist. However, BL claims that Kuhn is monistic, rigid and deterministic. Kuhn's theory is credited to saving the social sciences and freeing them from the hegemony of the natural sciences. Instead, BL claims Kuhn's theory is unsuitable for information systems and other social sciences because it follows the pattern of progress of the natural sciences, specifically of physics. What is the source of these misunderstandings? A reading of BL by itself will not uncover the source of such differing opinions. The inspiration behind BL can be found in Richard Whitley's (1984) *The Intellectual and Social Organization of the Sciences*. In this highly cited work, Whitley (1984) attempted to construct his sociological dimension in the progress of science, essentially his own version of the sociology of science. In the process, his work added yet another misunderstood interpretation of Kuhn's theory, which was adopted by BL in their recommendations for the IS field.

Whitley's Conception of Kuhn's Theory

Whitley's (1984) conception of Kuhn's theory is molded by his interpretation of its application in research policy, in particular, as it takes the form of the "Finalization Thesis" by the Starnberg group in Germany in the early 1970s. The Starnberg group featured prominently in Whitley's criticism of Kuhn even though the group's application itself diverged from Kuhn's theory. At the time, the Max Planck Institute for the Study of Living Conditions in the Industrial World, based in Starnberg, Germany, initiated a discussion on how society was to benefit from scientific research (Pfetsch 1979; Schopman 1980). In building their thesis, they applied part of Kuhn's theory to propose a three-stage model of scientific growth: (1) pre-paradigmatic stage, (2) paradigmatic stage and (3) post-paradigmatic stage. The first pre-paradigmatic stage is represented by the scientific community's efforts in collecting data and building theories, followed by a paradigmatic stage when a dominant theory becomes the paradigm, causing the field to become a "normal science." Up to this point the group remained faithful to Kuhn's ideas. It is in the third

phase where the group diverges from Kuhn's model of progress. Whereas Kuhn's model focuses on the anomalies that can be expected to occur during normal science, hence, resulting in crises and revolution, the Starnberg group expanded on the notion of a third post-paradigmatic phase. According to the Starnberg group, three major notions characterize this third phase, (1) a sense of completeness in research, from which the term the "Finalization Thesis" was derived, is reached, where all fundamental problems in the research area are resolved and puzzle-solving in the Kuhnian sense becomes routine, (2) instead of *discovery*, the third phase is characterized by *application* of the "completed" research to benefit society, and (3) a change from an internal autonomous force guiding the direction of research, to an external societal force planning and directing science. This third politically charged characteristic became a thorn on the side of the side of the Starnberg group's thesis because it implied a loss of autonomy for the scientists and a leaning to a more leftist stance of government control. This internal versus external struggle was the central argument used by Whitley to suggest that although Kuhn's model was influential in the move from a traditional philosophical view of science to a more sociological view, it also promoted a rather Orwellian view of science as a national resource that needed to be managed and controlled.

It is at this point that Whitley's assessment appears less coherent. In the beginning, he gives credit to Kuhn for transforming the view of science from "an epistemologically rational process" (p. 1) to one "to treat the natural sciences less reverentially and more of a social phenomena" (p. 2). However, in the next passage, he accuses Kuhn of superimposing a "unitary view of the 'mature' sciences and his apparent belief that once a field became 'mature' through unspecified processes, it follows the same pattern of intellectual change regardless of its external circumstances" (p. 3). Whitley continues by saying that Kuhn's model "prevented social scientists from studying variations between the sciences and investigating the social conditions in which radical intellectual change will occur" (p.3) presumably because the Kuhnian view treats all the sciences in the same manner. A cursory review of Kuhn's *Structure* is sufficient to show nothing of the sort, instead, if anything, *Structure* supports Whitley's claims that scientific progress is historically contingent (Kuhn was a historian after all) and largely sociological rather than epistemological. Whitley's renunciations are better suited towards Popperian views rather than Kuhn's, because it was Popper (1959) who insisted on the logical and epistemological source for scientific discoveries. In the end, it was Whitley's perception of the rigid and unitary pattern of Kuhn's model that made its way into BL's interpretation of Kuhn.

The Vague Nature of the Paradigm

The first critique of Kuhn in BL focuses on the vague notion of the paradigm concept. This part of Kuhn's theory is the most well known and not surprisingly, the most criticized (Shapere 1964; Shapere 1971). BL's contentions concerning the nature of the paradigm term can be categorized into three parts, (1) apparent multifarious meanings of "paradigm," (2) how IS researchers are likely to use the paradigm concept, and (3) its monistic implications. This section focuses on the first two. Kuhn's rather un-philosophical and expository style contributed to the vague meaning of paradigm. However, its expository mass appeal and vagueness were also the causes for its popularity, because in every conceivable domain, the Kuhnian paradigm was able to accommodate different meanings intended by its interlocutors. The difficulty of pinning down the exact meaning of the paradigm term did not diminish its usefulness and considerable impact in those numerous fields. All of this suggests that the term may not be as vague as its critics paint it to be. In many ways, the paradigm terms sits in the same category as other equally vague but nevertheless useful and critical terms such as "information" and "organization" which today have become integral parts of established theories.

In order to emphasize the multifarious meanings of the term "paradigm" BL enlists two of Kuhn's critics, Toulmin and Masterman. According to BL, Toulmin (1972) presents five distinguishable phases in Kuhn's use of this term between 1957 and 1970 implying five different meanings to the term. BL qualifies this statement by saying that "[T]hese phases are seen as being closely related to the changes in Kuhn's conception of revolution as it became more and more evident that he really meant evolution" (p. 49). There is an important difference between saying that the term "revolution" went through five different phases, which is what Toulmin meant, and saying that the term "paradigm" went through the same number of phases. Toulmin made it clear that "[T]he two key notions in Kuhn's account—'paradigm' and 'revolution'—are in fact separate and independent" (p. 106). So it would not be accurate to say that the term "paradigm" went through the same number of transformations as implied by BL. The term "paradigm" itself, according to Toulmin, is not a novel term and has been used in the past in the same way that Kuhn had applied it, to mean "conceptual system," "stereotypes," or "fundamental patterns of explanation" (p. 106-107), either in a limiting sense as Wittgenstein (Rhees 1968) had once used it or in a liberating sense as was used by Lichtenberg, a mid-18th century philosopher. Toulmin's contention was not about the multifarious nature of the term

"paradigm" but rather about Kuhn's thesis that paradigms experience an abrupt, discontinuous, or "revolutionary" transformation as part of scientific progress.

The second charge is the oft-cited claim by Masterman (1970) that Kuhn uses the term paradigm in no less than twenty-one difference senses. BL and others neglect to add that Masterman (1970), after listing all twenty-one senses, quickly concludes that they can be grouped into three main categories, (1) metaphysical paradigm, (2) sociological paradigm, and (3) construct (conceptual) paradigm. True to Kuhn's original interpretation, Masterman (1970) accepts the usefulness of the paradigm concept, especially in the period in which theories are absent (the pre-paradigm period), the paradigm becomes the guide by which scientists are still able to perform their research. Kuhn's critics who selectively choose parts of evidence that only support their contention often omit Masterman's positive evaluations of the paradigm concept. As in any intellectual dialogue, authors may modify their position as and when evidence support such a need. Kuhn did modify his interpretation of the paradigm concept as a result of the evidence presented to him, but the essential nature of the paradigm term remains useful and productive for research, and not as BL might imply—a confusing term to be held in contempt.

Perhaps BL's worst misrepresentation of the nature of the paradigm concept is the use of Marc De Mey's (1982) book, *The Cognitive Paradigm* to disparage applications of the paradigm concept. The title of the book is an obvious indication of De Mey's positive reception to the paradigm concept. Inside the book, De Mey defends Kuhn's paradigms in glowing terms and even acknowledges Masterman's (1970) positive appraisal of Kuhn's theories. However, BL selectively chooses a quote from De Mey (1982) to suggest that the paradigm concept is "so diffused, so much so, in fact, that De Mey [15, p. 104-105] classifies some of them in three groups labeled through "their activity as *paradigm-hunting, paradigm-detection* and *paradigm-dissection*. The paradigm-hunters are those 'enthusiastic Kuhn followers who hope to remedy a deplorable state in their field by providing a paradigm or by promoting the search for it.'" (p. 49). By highlighting only the first group, which De Mey (1982) contends is the least productive use of the paradigm concept; BL ignores De Mey's detailed description of the other two categories, which he considers are more productive applications of the paradigm concept. De Mey (1982) notes that paradigm-detection studies, which today take the form of scientometrics have "developed into an area that greatly contributes to our understanding of the social side of the paradigm concept" (p. 105). The concept of the "invisible college" (Price and Beaver 1966) is part of these paradigm-detection studies. Paradigm-dissection studies, which focus on understanding the mechanisms of the paradigm's functioning, became an inspiration to cognitive psychology and artificial intelligence. Cognitive historian Edwin Boring (1964) analogizes Kuhn's paradigm and incommensurability thesis to the notion of "cognitive dissonance" in cognitive psychology. Marvin Minsky (1975), among the pioneers of artificial intelligence, admit his debt to Kuhn for his frame theory. Minsky (1975) writes "The basic frame idea itself is not particularly original–it is in the tradition of the 'schema' of Bartlett and the 'paradigms' of Kuhn" (p. ..). Unfortunately, because of BL use of rhetorical devices and subtle misrepresented evidence, readers have since drawn disparaging conclusions about the paradigm concept.

Who's Paradigm Will Win?

Now that the nature and fecundity of the paradigm concept is demonstrated, and it is not an anathema as might be construed by reading BL, this section describes the second major criticism of Kuhn's model. BL's reading of *Structure* implies that Kuhn's model is a rigid, monistic model unsuited for a pluralistic and multi-disciplinary field like IS. BL bases their argument on Kuhn's (1970b, p. 19) statement "The new paradigm implies a new and more rigid definition of the field. Those unwilling or unable to accommodate their work to it must proceed in isolation or attach themselves to some other group." BL's mistaken reading is based on several false assumptions. First, it is based on the simplistic assumption that an emerging paradigm is chosen from existing competing paradigms. Second, only a single paradigm will triumph and hold sway over the other schools and paradigms, perhaps unjustly so. And third, in seeking this single paradigm, fields should spend time building such a paradigm in order to attain maturity. All these assumptions are implied in several passages in BL. Beginning with the last assumption, BL states that "one should not distract researchers from their daily activities and ask them to try to set up a set of rules to be called a paradigm; rather, one could observe how these researchers proceed, elaborate a model and propose it as a paradigm" (p. 50). Kuhn's writing does not in any way propose that researchers go searching for a paradigm in order to bring their fields to maturity. Rather, Kuhn propose that researchers continue to solve problems in their fields and it is the rules, theories and practices of the ones more successful in solving problems that will emerge as the new paradigm. BL is correct in saying that paradigms are *hic et nunc;* the result of the work of the scientists, not the result of the search for a paradigm, and in this sense, is in agreement with Kuhn. The second assumption implies a monistic conclusion to Kuhn's model because Kuhn apparently endorses only one triumphant paradigm. Following

Whitley (1984) and Chalmers (1998), this assumption excludes the possibility of several paradigms operating at the same time. Although Kuhn (1970b) insists on the importance of reaching that "first" paradigm in many instances, he makes it clear that the view of one total dominating paradigm is not the norm:

> What has been said so far may have seemed to imply that normal science is a single monolithic and unified enterprise that must stand or fall with any one of its paradigms as well as with all of them together. But science is obviously seldom or never like that (p. 49).

In the postscript of *Structure*, Kuhn asks "What do its members share that accounts for the relative fullness of their professional communication and the relative unanimity of their professional judgment? ... the answer, a paradigm or set of paradigms" (p. 182). The possibility that several competing schools may exists at the same time is significant because as Masterman (1970) correctly observes, the psychological, social and information sciences do not suffer from a lack of a paradigm, but rather, too many paradigms.

The first assumption takes a generous amount of space in BL's argument. After emphasizing the pluralistic nature of IS, BL predicts that "[E]ach researcher would bring along concepts and methods from his background discipline and, often, would continue doing research which is closely related to it" and:

> If a paradigm is to specify, as expected, the way in which research is to be conducted as well as how results are to be interpreted, then it seems dubious that we could end up with a paradigm that could include the different approaches currently found in the MIS field. A large number of MIS members would face the choice of either leaving or converting (in its religious sense) to the emergent paradigm. (p. 49).

BL's conclusion is based on the assumption of a one-to-one relationship between the scientific community and the scientific subject matter. In other words, multidisciplinary fields like MIS will need to decide on adopting the paradigm of one its referent disciplines (e.g. computer science, decision science, management, economics or psychology) in order to mature, which according to BL, will result in a "break-up of the field into rather hermetic factions and the consequent loss of the creativity generated by exchanges about research topics and research methods" (p. 51). Kuhn noticed the appeal of this notion and decided to address it in the postscript of *Structure*. The emergence of paradigms and intellectual fields do not progress in this manner. As Kuhn (p. 179) emphasizes:

> What is today the subject matter for a single broad community has been variously distributed among diverse communities in the past. Other narrower subjects, for example, heat and the theory of matter, have existed for long periods without becoming the special province of any single scientific community.

Consequently, the emergence of a paradigm is not the result of one subject matter overpowering another, or one competing school decimating the influence of another, but rather, several threads of thought or discourses synthesizing and providing solutions to relevant problems. This process offers solutions to the whole community, not just for certain parties at the expense of others. In order to understand how these complex processes work within the scientific community, the changing community structure over time needs to be unraveled, which is the focus of the so-called paradigm detection studies.

The Banality of Normal Science

The final criticism that will be examined is related to the nature of "normal science" in Kuhn's (1970b) model and the alleged banal and prosaic characteristic of this paradigmatic phase. BL questions the value of the "relative calm" in the absence of crisis. Why would any field want to work hard towards acquiring a paradigm when the normal science associated with this paradigmatic phase is characterized by "mopping-up operations and puzzle-solving that produce no major novelties" (p. 51) and does not guarantee progress? This perception of normal science cannot be further from Kuhn's model. Although Kuhn admits that research can proceed without paradigms (p. 11), all the facts collected during that period would all seem equally relevant because there is no set of agreed beliefs on which any evaluation can be compared. Second, with the establishment of a paradigm, the scientific community no longer needs to build the field anew, which is a common disadvantage for students in pre-paradigmatic fields. Therefore, students of paradigmatic fields can focus on the most esoteric aspects (the research front) instead of wasting time on building the research from first principles. Despite BL's contention that "normal science" is uninteresting, many will disagree and find their calling in these puzzle-solving activities. Additionally, it is also during these periods of stability that the professional aspects of the field have the most opportunities to grow and influence society, a fact

that the IS field would savor if it could. Although on the surface, the activities of normal science might be described as "mopping-up," it is these activities that allow scientists to investigate part of the field in enough "detail and depth, which would otherwise be unimaginable" (p. 24). The paradigm also guides researchers towards problems that are likely to have solutions because paradigms come "with a criterion for choosing problems" (p. 37). Whatever the motivation for the scientists, whether to be useful, to explore exciting new areas, to find order in apparent chaos, or to establish knowledge, the overriding motivation for most scientists is to solve a puzzle that no one has solved before, within the rules set by the operating paradigm.

The Legacy of "Can the Field of MIS Be Disciplined?"

The case of throwing out the baby (paradigm) with the bath water

The critical analysis of BL above uncovers several implications that have had significant impacts on how the IS community views their field. Whitley's (1984) analysis of Kuhn's model, which forms the basic foundations for BL, as "inevitable," "self-sufficient," "epistemological," "homogenous, totalistic, and coherent scientific communities dominating the lives and thoughts of scientists" (pp. 3-5) justifies his recommendation for a more sociological study of science, and supports BL's argument that Kuhn's model is rigid and totalitarian. It is beyond the scope of this paper to delve in depth on the merits of these pronouncements. However, what suffered the most after BL was the IS community's view of the paradigm concept. Even though the arguments brought by BL to discredit the paradigm concept were at best confused, and at worse, a misrepresentation, the legacy of BL has been to turn this transformative concept into an anathema to the IS community. Consequently, the community lost whatever benefits it might have brought the IS field, as history demonstrated it did for other fields. Many of the social sciences benefitted immensely from the paradigm concept. Friedrich's (1970) *A Sociology of Sociology* and Ritzer's (1980) *Sociology: A Multiparadigm Science* contributed to a redefinition of sociology as a multi-paradigmatic field and to free itself from the Parsonian functionalist paradigm. Interestingly, BL recruited Ritzer (BL p. 49) to support their argument against Kuhn. The sociology of technology studies was able to escape the clutches of the natural sciences, which, through political support, were grooming that field to become their foot soldier in accomplishing their model of scientific progress. As a result of Kuhn's *Structure* top universities around the world including the University of California Berkeley and Cornell University found policy-oriented programs that brought multiple perspectives from historians of science and philosophers to create the new Science and Technology Studies (Sismondo 2003). The sociology of knowledge and scientometrics, both owe their debt to Kuhn (Merton 1973; Price 1963). The social sciences found an ally in Kuhn, even though, Kuhn himself admitted that he knew nothing about the social sciences. He is quoted to have said "I know a great deal less than I should, and in any case virtually nothing at all, about the social sciences and I will not create confusion by bluffing it" (Deutsch et al. 1986, p. 278). Regrettably, like BL, many social scientists take Kuhn's observation of their field as deep criticism (Fuller 2000). And in the case of the IS field, the persuasiveness of BL's argument was such that very few paradigm-related studies [a notable exception was Goles and Hirschheim (2000)] were published afterwards because the "paradigm term has fallen into disrepute" (Ein-Dor and Segev 1981, p. vii), and of those few, only the brave ones dared to use the "paradigm" term in the title of their publications.

External versus internal focus on progress

BL and Whitley's interpretation of Kuhn's model promotes an externalist view of progress where maturity and legitimacy is more likely to come from close collaboration with other established fields rather than an internal agreement among the IS community itself. Alluding to the alleged lack of novelty and "restricted progress" of normal science, BL implies that internal efforts of putting the IS house in order "rests on shaky ground" (p. 51) and is viewed as one group imposing its views on another. BL adds, "we should not be so sure that the relative calm of a paradigmatic period is a lot better than the turmoil of a crisis" (p. 50), and in fact choosing order is equivalent to "possible sclerosis" (p. 50). They propose a free-for-all attitude and a "let many flowers bloom" approach to intellectual progress. At the very least, the legacy of BL in this regard is a near abandonment of efforts towards conceptual development within the IS field. A discussion of rules, explicit definitions of concepts, values and models, which are essential elements of a paradigm, is pushed to the background behind the next new theory borrowed from referent disciplines. "Letting many flowers bloom" is not a call for a free-for-all attitude because this is same argument that the social sciences had received from Kuhn in the sense of "let a thousand paradigms bloom" (Fuller 2000, p. 245). Because a paradigm implies that a unit of knowledge production such as a field can be conducted largely in relative isolation from other knowledge producers, the field therefore becomes autonomous.

What is supposed to take place is the redefinition of each field according to their own set of rules and values without necessarily depending on parent or more influential disciplines, as is the current predisposition with the IS field. The sad legacy of abandoning the paradigm concept is the continued dependence of the IS field on its parent disciplines.

The competing schools within IS

The final legacy of BL can be described as the "either-or" thesis of competing schools within IS; either I win or you win. An extension of the previous legacy, which promotes communication from outside the field as opposed to communication within the field, is the fear that one school of thought or paradigm will dominate at the expense of others. For fear of this happening, the IS community continues to welcome every field into its fold, but unfortunately leaves them to their own devices without actively asking them to engage other existing schools of thought. As a result, bridging the gap between the technical and the behavioral in IS remains illusive. The technological groups are left to hold their conferences, (e.g. WITS) while those leaning towards the human sciences, which make up perhaps the larger majority in IS, continue to borrow from their fields to address technological issues. Ironically, BL's warning about the "break-up of the field into rather hermetic factions" (p. 51) becomes a self-fulfilling prophecy. The IS field is unwilling to embrace the paradigm concept because it is fearful of fragmenting it, but it is the fear itself that is maintaining the field's fragmentation.

Conclusion: Practical Implications and Future Research

Harvesting the power of the paradigm concept

Fortunately, a few IS authors continued applying the paradigm concept in their research despite BL's insistence to the contrary. Goles and Hirschheim (2000) introduced the pragmatic approach to IS research using the sociological paradigms of Burrell and Morgan (1979). Iivari (1991) organized the very messy jungle of IS development methods using a paradigmatic analysis based on their ontology, epistemology, methodology and ethics. The fluid nature of the paradigm concept allows researchers to study different subject matter areas without fragmenting the field. The IFIP conferences (Mumford et al. 1985; Nissen et al. 1991) introduced the IS field to alternative research paradigms and allowed the IS field to incorporate different approaches into its methods (Mingers 2004). What has not been explored is the power of the paradigm concept in organizing the fragmented subject matter of the IS field. In the same way that the natural sciences explored the nature of light, switching from corpuscular theory to wave theory to finally settling on something in between (Kuhn 1970b), the paradigm concept is a powerful way of organizing the subject matter of the objects being studied. The history of the IS field can basically be organized into several paradigmatic phases representing the objects of study beginning with the decision making paradigm of the early 1970s to the strategic management (or competitive advantage) paradigm of the 1980s and the organizational (or business process) paradigms of the 1990s. These paradigms carry with them a common thread that links what might appear to be disparate subject matter areas to the IS field. Arguably, beginning in the early 1990s, several paradigms began dominating the IS field's attention, specifically, the social psychology paradigm of IT acceptance and adoption (Davis 1989), the ontological (Wand and Weber 1995) and the artifactual paradigms (Weber 2003). All of these topics are interesting subject matter areas. The question is, which of these, if any is capable of enhancing its relevancy to society and progressing the IS field forward? The paradigm concept opens the IS community to other subject matter areas that may be outside the comfort zone of the IS community. The artifactual paradigm evolved into a design science paradigm (Hevner et al. 2004) focusing on issues surrounding the development of the IT artifact itself. The sociological paradigm introduced to the field by Goles and Hirschheim (2000) have not been thoroughly explored. Other paradigms such as the network paradigm are just now beginning to be explored (Boland et al. 2006). Other paradigms quite relevant to the study of IS, such as the cognitive paradigm, are yet to be explored. The paradigm concept opens the IS field to a vast array of subject matter while at the same time maintaining the field's cogency.

Growing from within: A moratorium on borrowing

Evidence for a growing and maturing field is its ability to grow from within and to exert an influence on other fields (Baskerville and Myers 2002; Foucault 1972; Nerur et al. 2005). Unfortunately, the IS field continues to borrow incessantly from other disciplines (Nerur et al. 2006) and arguably does not exert much influence on them (Wade et al. 2006). Consequently, the growth of the field has been largely the result of direct and uncritical borrowing of

concepts and theories from other disciplines (Markus and Saunders 2007). The paradigm concept offers the epistemological basis for a critical and novel way of borrowing. As a metaphor or model, the paradigm concept enables a researcher to describe an unfamiliar object of study using one that is already well known. Mary Hesse (Hesse 1967, p. 356), Oxford philosopher explains this power of the paradigm or theoretical model:

> Basically, the theoretical model exploits some other system … that is already well known and understood in order to explain the less well established system under investigation … the theoretical model carries with it what has been called "open texture" or "surplus meaning," derived from the familiar system. The theoretical model conveys associations and implications that are not completely specifiable and that may be transferred by analogy…"

An example of exploiting an already familiar system is the model of the "wheel" (Kaplan 2001). In its specified form, the wheel as a machine that turns around and forms the necessary part of a moving vehicle is well known. In its paradigmatic form, the wheel as a theoretical model has a great deal of plasticity and "surplus meaning" that allows people to use it to explain other phenomena. The expressions, the "wheel of life" and the "wheel of fortune" exploit the constant turning of the wheel bringing change to our lives in unexpected ways. The expression "A fifth wheel" explains yet another aspect of the wheel implying an image of something unnecessary since customarily, four wheels is sufficient for any vehicle. In each case, although the qualities and models from the wheel are borrowed, no details are assumed, and each expression takes on its own novel meaning and varied implications. The same process has been historically the hallmark of scientific progress. The discovery of oxygen and the nature of light depended on the generous borrowing of metaphors and models from other more familiar phenomena without necessarily picking up any specific detail (Kuhn 1970b). In the social sciences, biological metaphors form the basis for major theories of sociology (Spencer 1897). In the same way, as a multidisciplinary field, IS has available to it a wealth of metaphors and models from other disciplines. At the paradigmatic level these metaphors and models provide for a rich source for inventive conceptual activity without the stigma and limitations of direct borrowing.

Serious engagement based on the paradigm concept

The paradigm concept offers future IS research the necessary tools for a serious engagement with its issues. The illusive gap between the technical and the behavioral need not remain. The artificially created dichotomy between the IT artifact and its contextual environment holds the IS field hostage to other disciplines. As a result of this dependency, IS scholars dabble in psychology, sociology, economics, marketing, engineering, management and other disciplines without necessary expanding their horizons in either the IS field or the discipline that defines the contextual environment. All established disciplines go through the same process of freeing themselves from their parent disciplines without necessarily severing all ties. This process of extricating itself from its parent disciplines is a necessary step before it can seriously engage with its own issues. Physics and biology had to extricate themselves from natural philosophy (Foucault 1970) before becoming disciplines in their own right. Political science emerged from history (Manicas 1987). Biochemistry found its calling by finding a common ground between what constitutes life (biology) and the science of inanimate matter (chemistry) (Whewell 1858). Each disparate subject matter area in the IS field need to find the common ground with other subject matter that binds them to the unique IS discourse. It is at the intersection of these varying subject matter areas that the IS field is most likely to find itself and hence its *raison detre*.

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